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Mark, Edward Laurens
Trichinae in swine

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TRICHINÆ IN SWINE.

By PROFESSOR E. L. MARK, of Harvard University.

1889.

I began my examination of the hogs killed in the vicinity of Boston in the autumn of 1881. It was then thought that it might prove to be desirable to carry on investigations which could be better conducted at Chicago than in this vicinity. My first examinations were, therefore, confined mostly to so-called Chicago hogs; *i. e.*, such as were collected west of Chicago, and shipped to slaughterers from that point. But toward the end of the examinations made in that year, and not included in the five hundred reported upon, there was one small lot of hogs raised near Boston which contained two trichinous individuals. That circumstance suggested the possibility that the source of trichinæ among American hogs might possibly be sought to advantage here, instead of going to the West. The matter remained, however, without anything further being done by way of examinations until 1883-84, when a renewed interest in the subject was awakened.

When examinations were resumed in 1884, it was upon a somewhat different plan from that pursued at first. It seemed improbable that success could be attained by any other means than a careful study of all the conditions under which the hogs were raised. I had found that it would be practically impossible to trace back the history of hogs shipped from Chicago and other Western cities, because they were generally collected over extensive territories, and no record was kept of their origin. It therefore seemed best to limit examinations to animals which were *raised* in this immediate vicinity. The conditions under which they were reared

could certainly be more readily inquired into than in the case of Western hogs. The principal difficulty in the way of this plan was the limited number which one was likely to get for examination. It was also thought best to continue the examination for a number of years, so that the results should not be influenced by any possible yearly fluctuations in the prevalence of the trouble.

The examinations had not progressed very far before it became evident that there was a degree of infection in the hogs raised near Boston which considerably exceeded anything hitherto known. At the same time my attention was naturally directed to the nature of the feed which they received. I soon learned by inquiry that many of the raisers fed their hogs largely on city offal. I hoped that the material which came to me for examination would embrace many raisers who did not, as well as many who did, feed city offal; and that it would be possible, by a comparison of the results in the two classes of cases, to arrive at a definite opinion as to whether the offal was or was not responsible for the large proportion of trichinous hogs. In this I have been disappointed, inasmuch as nearly all the raisers—51 out of 56—whose hogs I have examined to the number of fifteen or more have made use of offal. While this prevents the presentation of comparative statistics which would be of value, it in no way diminishes my apprehension that city offal may be largely responsible for the prevalence of trichinæ here. At least, I shall not be satisfied that city offal is free from danger until it has been established by suitable experiments that the proportion of trichinous hogs cannot be reduced by wholly excluding it from their feed.

In the supplement to the first annual report of the State Board of Health, Lunacy and Charity of Massachusetts (1879, page 38) were published the results of the examinations for trichinæ in swine, made by Dr. F. S. Billings, V. S. Out of the 2,701 hogs examined, 154 were trichinous, the ratio of trichinous to non-trichinous hogs being 1:17.54; stated in another form, about 5.7 per cent. were trichinous. Of these, only 12 were raised in the vicinity of Boston. In the third annual report of the State Board of Health, Lunacy and Charity (page xlvi) the results of

the examination by Dr. Billings of 6,068 additional hogs, mostly from the Western States, showed the per cent. to be somewhat less than in the first report. Only 191 out of the 6,068 were infected; *i. e.*, 1 in every 31.77, or 3.15 per cent.

During October and November, 1881, I examined thoroughly *the pillars of the diaphragm only* from 500 hogs. The results have been given by Dr. Abbott in the supplement to the fifth annual report of the Board (page 182). Of the 500 hogs, 10, or 2 per cent., were found to be trichinous. In each case nine slips of meat from one "pillar" were examined. Three of the ten cases were only slightly infected. Had the examination been restricted to the first three or even to the first six slips, instead of nine, the result would have been negative, since the single specimen of trichina observed was found in the ninth, the eighth, and the ninth slips respectively in the three cases. One of the specimens of meat was further tested to ascertain the proportion of trichinæ in relation to the number of slips. Thirty slips in all were examined, and only five trichinæ were found; so that, had the worms been *evenly distributed* through the muscles, one would have found a single worm in each group of six slips. The possibility of overlooking the existence of trichinæ in such a mild case as this, even with the examination of six slips, is evident, since it cannot be assumed that the worms are evenly distributed through the muscle. Observation has shown repeatedly that the reverse is true; not only that different muscles but also that different parts of the same muscle may be infected in very different degrees.

I have dwelt at some length upon these examinations, because in nearly all subsequent examinations only *six* slips were employed; and it is therefore evident that some correction should be applied to the results, in order to satisfy the probability that the trichinæ of certain specimens have been overlooked, owing to their infrequency and the small number of slips examined. It would be quite unfair to infer, however, that the results which I have arrived at should be corrected by adding *three* to every seven found by the examination of six slips to be trichinous. For, in the first place, I believe, from the results of other observations, com-

plete records of which were not kept, that three such cases in ten is an unusually large proportion; and, secondly, if it were certain that out of every ten trichinous hogs three were so feebly infected that nine slips must be prepared to secure one trichina, there is no reason to suppose that the slip containing the single trichina would be the seventh, eighth and ninth, any more than the first, second and third; so that, if six slips were examined, the chances are that in two-thirds of these cases the trichina would be found. Thus, at most, there would have to be added *one* — not three — to each seven found trichinous, in order to counterbalance the probable oversight. But, as I have said, I believe that this is much too large a correction. One to ten, or even one to twenty, would probably be much nearer the reality. Supposing, however, that for every ten found by such examination to be trichinous one were to be added; even that could have very little practical importance, especially when those which are shown positively to be infected reach such high numbers as appear in this report. It is in view of the comparative insignificance of this error that I have ventured to limit my examinations to six slips, and have concluded that the degree of accuracy attained was sufficient for all practical purposes. It is to be remembered that this series of examinations has been undertaken not as an *inspection* of pork, where the examination should be rigid and as nearly absolute in its results as possible, but simply as a means of finding an approximate answer to the question, What proportion of the hogs raised in a given district are trichinous?

In order that the examination should be as thorough as possible under these conditions, the slips were taken each from a different part of the "pillar," so that no considerable portion of the specimen remained without inspection. The six slips together weighed on the average about twelve centigrams (one grain); they were cut in the usual manner, with scissors, laid on a glass slide, and covered by a slightly narrower slide of thinner glass. The cover-slide was ruled lengthwise with parallel lines, slightly closer together than the diameter of the field of the microscope, to aid in counting the number of trichinae in the infected specimens. The cover-slide was applied with the ruled side down; that is, in

contact with the meat. In counting, only the worms embraced between two lines were considered while the slide was being moved in one direction; when the end of the slide was reached, the next adjoining space between two lines was observed while the slide was slowly moved in the opposite direction. In this way the whole slide was accurately examined without danger of counting more than once any single specimen of the worm. The meat was of course firmly pressed between the two slides, so as to flatten it and make it transparent; and a slight pressure was continued by means of the thumb and finger with which the slide was grasped in moving it back and forth.

The magnifying power employed was usually thirty or forty-five diameters. In addition to noting the specimens of meat which were infected with trichinæ, and also the number (approximately) of worms found in each case, a record was kept of the *sex of the hogs* on which the examinations were conducted, so that the results show the proportion both of males and females that were found to be trichinous.

TABLE I. — Summary of Results of Examinations for *Trichina* made on Hogs raised near Boston, 1883-88.

DATES.	Serial Numbers.	NO. OF HOGS EXAMINED.			NO. OF HOGS TRICHINOUS.			PER CENT. TRICHINOUS.			REMARKS.
		M.	F.	M. and F.	M.	F.	M. and F.	M.	F.	M. and F.	
1883-84, . . .	561-1109 { (less 302), }	141	105	246	18	22	40	12.77	20.95	16.26	The 302 foreign hogs were numbers 638-665, 693-743, 764-813, 860-982, 992-1046.
1884-85, . . .	1109-1984, . . .	527	348	875	61	23	84	11.57	6.61	9.60	
1885-86, . . .	1984-2656, . . .	490	182	672	80	23	103	16.33	12.64	15.33	
1886-87, . . .	2656-3271, . . .	390	225	615	59	24	83	15.13	10.67	13.50	
1887-88, . . .	3271-3927, . . .	413	243	656	58	26	84	14.04	10.70	12.80	
1883-88, . . .	Totals, . . .	1,961	1,103	3,064	276	118	394	14.07	10.61	12.86	Embraces 32 hogs, fed on city swill, from Manchester, N. H.

TABLE II. — *Summary of Results of Examinations for Trichinæ made on Hogs from State Institutions, 1884-88.*

DATES.	SERIAL NUMBERS.	Number Examined.	Number Trichinous.	Per cent. Trichinous.
1884-85,	12020-12109 (less 4), . . .	85	11	12.94
1885-86,	12111-12163,	52	14	26.92
1886-87,	12163-12227,	64	11	17.19
1887-88,	12227-12260,	33	6	18.18
1884-88,	Total State Institutions, . . .	234	42	17.95
1883-88,	Total near Boston,	3,064	394	12.86
1883-88,	Total Massachusetts,	3,298	436	13.22

Index List of Fifty-six Raisers and Eight State Institutions.*

[Tables III. and IV.]

Ser. No.	LOCATION.	Ser. No.	LOCATION.
1, . . .	Bunker Hill.	34, . . .	Hingham.
2, . . .	South Boston.	35, . . .	Milton.
3, . . .	Milton-Randolph.	36, . . .	Tewksbury.
4, . . .	Newton.	37, . . .	West Roxbury.
5, . . .	Medford.	38, . . .	Revere.
6, . . .	Canton.	39, . . .	Canton.
7, . . .	Malden.	40, . . .	Dedham.
8, . . .	Watertown.	41, . . .	Bunker Hill.
9, . . .	Arlington.	42, . . .	Arlington.
10, . . .	Woburn.	43, . . .	Arlington.
11, . . .	Arlington.	44, . . .	Canton.
12, . . .	Milton.	45, . . .	Randolph.
13, . . .	Canton.	46, . . .	Somerville.
14, . . .	Brighton.	47, . . .	Arlington.
15, . . .	South Braintree.	48, . . .	Dedham.
16, . . .	Mattapan.	49, . . .	Weston.
17, . . .	Hingham.	50, . . .	West Roxbury.
18, . . .	Mattapan.	51, . . .	Arlington.
19, . . .	Waltham.	52, . . .	Holbrook.
20, . . .	East Cambridge.	53, . . .	Weston.
21, . . .	Randolph.	54, . . .	Weston.
22, . . .	Hingham.	55, . . .	East Braintree.
23, . . .	West Roxbury.*	56, . . .	Hingham.
24, . . .	Everett.		
25, . . .	Somerville.	1, . . .	Danvers Lunatic Hospital.
26, . . .	Belmont.	2, . . .	Lancaster Industrial School.
27, . . .	Charlestown.	3, . . .	Monson Primary School.
28, . . .	Manchester, N. H.	4, . . .	Northampton Lunatic Hospital.
29, . . .	South Boston.	5, . . .	Taunton Lunatic Hospital.
30, . . .	Canton.	6, . . .	Tewksbury Almshouse.
31, . . .	Dedham.	7, . . .	Westborough Reform School.
32, . . .	Hingham.	8, . . .	Worcester Lunatic Hospital.
33, . . .	Quincy.		

* The names of the raisers are withheld.

TABLE III.—*Results of Examinations, tabulated by Years, for all Raisers near Boston who have furnished Fifteen or more Hogs.*

Serial No.	'83-8.	'83-4.	'84-5.	'85-6.	'86-7.	'87-8.	Serial No.	'83-8.	'83-4.	'84-5.	'85-6.	'86-7.	'87-8.
1,	20:7	-	-	-	-	20:7	30,	23:3	5:0	7:1	5:1	-	6:1
2,	17:0	-	17:0	-	-	-	31,	35:9	-	-	13:5	17:4	5:0
3,	82:9	15:2	24:1	22:5	12:0	9:1	32,	43:1	26:1	1:0	1:0	5:0	10:0
4,	15:0	-	3:0	4:0	5:0	3:0	33,	16:3	-	4:2	-	6:1	6:0
5,	18:1	-	18:1	-	-	-	34,	41:10	-	-	-	17:6	24:4
9,	22:4	-	5:0	-	4:1	13:3	35,	21:3	-	10:1	11:2	-	-
7,	27:3	10:2	1:0	16:1	-	-	36,	65:2	-	65:2	-	-	-
8,	17:1	7:0	-	-	-	10:1	37,	35:4	-	-	35:4	-	-
9,	27:3	-	8:0	15:3	3:0	1:0	38,	27:5	-	-	-	27:5	-
10,	101:11	-	29:1	44:8	17:1	11:1	39,	19:1	9:1	4:0	3:0	3:0	-
11,	22:2	-	22:2	-	-	-	40,	19:3	-	-	-	16:3	3:0
12,	41:12	12:5	29:7	-	-	-	41,	16:3	-	6:0	-	3:2	7:1
13,	85:9	-	42:3	-	40:4	13:2	42,	40:9	-	-	-	40:9	-
14,	45:1	-	10:0	35:1	-	-	43,	20:3	(1881) 11:2	9:1	-	-	-
15,	16:3	-	-	16:3	-	-	44,	44:6	-	10:3	-	6:0	28:3
16,	67:16	-	26:7	2:0	15:5	24:4	45,	30:3	-	-	12:1	18:2	-
17,	33:4	-	-	-	18:2	15:2	46,	23:1	-	23:1	-	-	-
18,	20:7	5:3	5:1	7:1	3:2	-	47,	24:1	7:0	17:1	-	-	-
19,	42:5	-	8:0	25:3	9:2	-	48,	20:2	-	-	20:2	-	-
20,	16:2	4:0	4:0	4:2	4:0	-	49,	22:5	-	-	15:4	-	7:1
21,	55:10	20:5	12:3	-	5:0	18:2	50,	18:0	-	18:0	-	-	-
22,	40:5	-	10:1	16:2	4:0	10:2	51,	24:3	-	-	15:3	3:0	6:0
23,	25:5	-	-	-	-	25:5	52,	16:0	-	-	-	-	16:0
24,	22:5	7:3	6:1	4:1	-	5:0	53,	15:1	-	3:0	-	-	12:1
25,	15:4	-	1:0	-	14:4	-	54,	16:0	-	16:0	-	-	-
26,	47:9	-	-	-	47:9	-	55,	18:1	-	3:1	15:0	-	-
27,	25:2	-	13:2	4:0	4:0	4:0	56,	37:3	-	-	-	37:3	-
28,	32:7	-	-	-	-	32:7	1767:246 = 13.92 per cent.						
29,	46:14	-	-	-	46:14	-							

TABLE IV.—*Results of Examinations, tabulated by Years, for all State Institutions from which Material has been received.*

Serial Numbers.	'83-8.	'83-4.	'84-5.	'85-6.	'86-7.	'87-8.
1,	7:2	-	7:2	-	-	-
2,	6:0	-	3:0	1:0	2:0	-
3,	22:1	-	7:0	15:1	-	-
4,	137:35	-	31:8	16:10	57:11	33:6
5,	18:4	-	6:1	12:3	-	-
6,	36:0	-	25:0	8:0	3:0	-
7,	[65:2]*	-	[65:2]		3:0	-
8,	5:0	-	3:0	-	2:0	-
8,	3:0	-	3:0	-	-	-

* The numbers bracketed are the result of examinations on specimens which were not received directly from the institution, and are included under No. 36 of the preceding table. Omitting these, the totals are 234:42 = 17.95 per cent.

An inspection of the above tables shows that, of the 3,064 hogs raised near Boston, 394, or 12.86 per cent., were trichinous; and that, for the several years over which the examinations extended, the lowest per cent. of infection was 9.6, the highest 16.26.

As regards the sex of the hogs, there were nearly twice as many females (1,961) as males (1,103) examined. The degree of infection among females was 14.07 per cent., while in the males it was only 10.61 per cent. It is noticeable that there was greater fluctuation from year to year among the males (20.95 per cent. maximum; 6.61 per cent. minimum) than among the females. The per cent. with the latter never fell much below 12. I know of no grounds upon which the comparative immunity of the males from infection can be explained.

Only 234 hogs slaughtered by State institutions have been examined, but of these, 42, or 17.95 per cent., were trichinous. Over half of these were received from one institution, where specially dangerous conditions were discovered, which may serve to explain in part the high degree of infection. This case will be considered farther on. The combination of these two tables of results gives, as the total number of hogs examined from Massachusetts, 3,298, of which 436, or 13.22 per cent., were trichinous. Besides these, I have examined from all other sources — principally Western, and *including the 500 reported on in 1881* — 888 hogs, of which 21, or 2.36 per cent., were trichinous.

Table III. gives the results of examinations in fifty-six cases, tabulated by years. They embrace all the cases of hogs raised near Boston in which as many as fifteen hogs from the same source have been secured for examination. The number before the colon shows the number of hogs examined; that after the colon, the number found to be trichinous. Table IV. gives similar statistics from eight State institutions.

In Table V.* is given the number of encapsuled worms found in each of the 436 cases discovered in hogs raised near Boston or in State institutions. The amount of muscle examined was subject to some variation, so that the figures

* A dagger (†) following a number indicates that the parasites were dead.

can be considered only as rough approximations to an index of the degree of infection, but on the average it was about one grain of flesh from each hog. A synopsis of the cases is as follows:—

With	1 parasite	87 cases	÷	1 = 87.*
“	2-5	“ 171	“ ÷	4 = 43.
“	6-10	“ 74	“ ÷	5 = 15.
“	11-19	“ 29	“ ÷	9 = 3.2
“	20-49	“ 36	“ ÷	30 = 1.2
“	50-99	“ 27	“ ÷	50 = .54
“	100-900	“ 12	“ ÷	800 = .015
	Total,	—		436

In 45 cases one or more of the parasites were dead.

What degree of infection is implied by finding only one trichina in six slips from a single hog? To find only one specimen of the parasite would seem, at first sight, to mean a degree of infection which is of little or no significance, either to the animal infected or to the consumer. But, however unimportant the infection may have been in the life of the hog thus feebly affected, even such cases are capable of producing serious results in the consumer. As has been said, the amount of meat examined from any one hog is excessively small—only about one grain. Since in a pound of meat there are 7,000 grains, it follows that, if one were to eat half a pound of such meat uncooked he would import into his person no less than 3,500 living worms, over half of which it has been proved would be productive females, each one of which would bring forth at a minimum about 1,500 minute worms, or, at a very low estimate, an aggregate of say two and one-half million worms. But, assuming that the average infection of the body falls considerably below that of the pillars of the diaphragm,—assume it to be one-fifth the latter,†—still the number of migrating young trichinæ (half a million) would doubtless be enough to have a real effect on the health of the patient.

* The figures of the last column are averages for the lots, obtained by dividing the number of cases by the number of grades embraced in the corresponding lot; thus, “2-5 parasites” embraces four grades—the 2- 3- 4- and 5- parasite grades.

† A little farther on there are presented some facts and estimates on the degree of infection.

But there is another reason why it is important to show the significance of what appear to be cases of feeble infection. I wish to know whether it is probable that the infection in these mild cases has been due to the adventitious importation of young trichinæ previously voided with the fæces of infected animals living in the same pen. One might naturally infer that all such mild cases came about simply by being associated with animals that were infected, and that it by no means implied the consumption of trichinous meat on the part of the mildly affected hog. But I believe that there is very little ground in support of such a theory, and that probably all, or at least very nearly all, the cases reported here imply the consumption of uncooked trichinous meat on the part of the infected animal.

It may be assumed that the weight of the lean meat of an average sized hog is 50 pounds,* or 350,000 grains. Since in each grain of flesh examined there was found at least one trichina, the mildest cases recorded would contain not less than 350,000 encapsuled worms, *if all the muscles of the body were as thoroughly infected as are the pillars of the diaphragm*. But observations have shown that that is not the case. Billings † has cited the results of the studies of Johow and Maximilian on the distribution of trichinæ in the hog, which may be summarized as follows. Specimens were taken from six muscles of each of four hogs. It is to be assumed — though it is not so stated by Billings — that the number of slips taken from each of the muscles was the same, for otherwise the results would have no significance in ascertaining the distribution of the trichinæ. The pillars of the diaphragm were considered separately from the other muscles of the diaphragm. The number of trichinæ found in each of the muscles is given in a table below (page 125). For the purpose in view, it is only necessary to ascertain the ratio of the worms found in the pillars of the diaphragm to the total number of worms found. This ratio, in the cases

* Leuckart ("Menschl. Parasiten," Bd. II., p. 532) assumes that the muscles of a person weigh forty pounds, and Huxley ("El. Lessons in Physiol.," Revised Ed., p. 365) puts the weight of the "muscles and their appurtenances," for a person weighing one hundred and fifty-four pounds, at sixty-three pounds.

† Supplement first annual report State Board of Health, Lunacy and Charity of Massachusetts, 1879 (pages 27, 28).

of the four hogs, was as follows: 12:17, 10:18, 40:85 and 40:98; or, combining all four, the ratio is 102:218. It therefore follows that the examined portions of the pillars contained nearly as many trichinæ as all the other portions of examined muscles put together, or nearly 50 per cent. of the worms discovered. But the *degree* of infection, neglecting for the moment the disparity in the weight of different muscles, is to be ascertained by dividing the number of parasites by the number of muscles in which they are found. The *average* degree of infection for all parts examined is therefore expressed by $(218 \div 6 =) 36$, while the degree of infection for the pillars of the diaphragm is expressed by $(102 \div 1 =) 102$. Consequently, the ratio* between the degree of infection in all the muscles examined and that in the pillars is 36:102, or approximately 1:3. But the muscles examined by Johow and Maximilian were principally those which are most liable to be infected. It would evidently be

* This ratio would not have been greatly altered had the "pillars" been regarded as a part of the diaphragm, provided the examinations of the whole diaphragm had resulted in finding the same number of trichinæ as the average of the results from pillars and diaphragm examined separately. This will appear from a comparison of the following table of their results, and computations based on the above assumptions:—

*Distribution of Trichinæ in Hogs, according to Johow and Maximilian.**

REGION EXAMINED.	NUMBER OF TRICHINÆ.					NUMBER OF TRICHINÆ (ASSUMED).				
	HOG.				Total.	HOG.				Total.
	No. 1	No. 2	No. 3	No. 4		No. 1	No. 2	No. 3	No. 4	
1. Pillars of diaphragm,	12	10	40	40	102	} 8	8	33	35	84
2. Muscles of diaphragm,	4	6	25	30						
3. Laryngeal muscles,	1	2	4	10		1	2	4	10	
4. Intercostal muscles,	0	0	6	10		0	0	6	10	
5. Tongue muscles,	0	0	8	6		0	0	8	6	
6. Neck, eye and humerus,	0	0	2	2		0	0	2	2	
Totals,	17	18	85	98	218	9	10	53	63	135

* The first series of four columns exhibits the results as given by Johow and Maximilian; the second series of four columns shows what the result would have been on the above assumptions. The ratio derived from the numbers 102 and 218 has been explained above. The ratio upon the new assumptions would be—dividing each total by the number of muscles furnishing the trichinæ— $(135 \div 5 =) 27$; $(84 \div 1 =) 84$, or 1:3.1; which is only slightly in excess of the ratio obtained when pillars and diaphragm are considered separately.

unfair, therefore, to put the average infection of the whole body as high as that of the muscles examined. Perhaps it would not be unreasonable to assume that the muscles less liable to infection—*e. g.*, those of the legs and back—would have been comparatively free from parasites; and since, on the whole, the muscles not examined greatly exceed in weight those which were, it would probably be not far from the truth to suppose that the including of such muscles in the examination would have reduced the average infection of the whole body to half the average infection of the muscles which were examined. Upon that assumption the pillars of the diaphragm would be six times as badly infected as the muscles of the body taken as a whole.

This result agrees fairly well with the examinations of Kühn, cited by Leuckart ("Untersuch. über *Trichina spiralis*," 1866, pp. 105, 106). Kühn examined fifteen different muscles from each of three hogs, with the total result that the diaphragm—not the pillars alone—contained 25 per cent. of all the worms found. Now, if the worms had been evenly distributed, he would have found in the diaphragm only ($100 \div 15 =$) $6\frac{2}{3}$ per cent., instead of 25 per cent. That is to say, the diaphragm is four times as badly infected as the average of all the muscles examined. Since the muscles examined by Kühn were not limited to those which usually show the larger proportion of trichinæ, it is fair to assume that the proportions which he found would hold good if every muscle of the body had been examined. That proportion is not very far from the results which the examinations of Johow and Maximilian would indicate, according to the view which I have taken of their investigation. In Kühn's case,* however, the fact that the muscles falling

* Kühn's results, which are slightly defective, from the omission of the ear-muscles and the extensors of the thigh,—they not having been uniformly examined,—are as follows:—

MUSCLES.	TRICHINÆ. Per cent.	MUSCLES.	TRICHINÆ. Per cent.
Diaphragm,	25.3	Eye,	3.6
Shoulder blade,	14.0	Belly,	3.6
Lumbar region,	11.3	Extensors, fore leg,	3.1
Larynx,	8.5	Nape,	2.6
Flexors, hind leg,	7.0	Flexors, fore leg,	2.5
Neck,	4.8	Intercostal,	1.7
Tongue,	4.7	Dorsal,	0.3
Cheek,	4.4		

below the average degree of infection ($6\frac{2}{3}$ per cent.) outweigh those which exceed it, would tend to diminish the average degree of infection for the whole body, and consequently to raise the ratio of the infection in the diaphragm to that of the whole body. I believe, then, in view of these two results, that one will not greatly err in assuming that the pillars of the diaphragm are at most five times as thoroughly infected as are the muscles of the body in general. But, even with this difference between the pillars and the other muscles of the body, there would still be in the mildest case here recorded not less than 70,000 encapsuled worms; a number which implies that they are the progeny of at least 46 fertile females, — reckoning the total offspring of each female at 1,500.

It is known that both embryos and gravid females are capable of living outside a host (*i. e.*, in fæces) only a short time, probably never more than twenty-four hours. (Leuckart, "Menschl. Parasiten," Bd. II., p. 558.)

That sexually mature females (*i. e.*, intestinal trichinæ), as well as embryos, are occasionally eliminated from the intestine of an infected pig, has been shown by a number of observers (Leuckart, Kühn, Vogel, Gerlach, *et al.* See Leuckart, "Menschl. Parasiten," Bd. II., p. 557); but it is still questionable whether the adult worms are able to withstand the action of the digestive fluids of the stomach when they are imported into a second host. Leuckart (*op. cit.*, p. 557-561) has discussed this point at some length; and, not only from the absence of satisfactory proof in the case of trichinæ, but also on general grounds, doubts the possibility of infection in that manner. If the sexually mature females of trichinæ were capable of withstanding the digestive fluids of a new host, they would form an exception, he says, to all other helminths, none of which are capable of enduring *at that stage of their existence* a change of host. I believe, then, that one is safe in assuming that a degree of infection which implies the importation of 46 gravid females precludes the explanation suggested. The importation of 70,000 separate embryos through fæces seems to me to be an equally unreasonable assumption, even admitting that the embryos are more capable than adults of resisting the digestive fluids in the stomach of the second host.

But there is still another, and, one must admit, more probable, source of infection through the contents of the fæces. Leuckart (*op. cit.*, p. 561) calls attention to the fact that, up to the third day after feeding with trichinous flesh, the intestines of the animal experimented upon may contain fragments of meat which are incompletely digested, and which may still contain encapsuled trichinæ in a more or less normal condition. Occasionally, he adds, such fragments may be found in the fæces. Of course, the importation of such fragments derived from the fæces would be just as dangerous as the eating of so much meat which had not passed through the intestine of another animal; besides, in that condition, — more or less protected from desiccation by being enveloped in the fæces, — the encapsuled trichinæ might survive for a long time. But I cannot think that anything except an unusual combination of circumstances would lead to so extensive an infection even by this means.

Taking all these facts into account, I do not believe that any considerable proportion of even the mildest cases reported here can be accounted for on any other assumption than that of direct infection by the eating of meat containing encapsuled trichinæ. But, granting what certainly is possible, that a very small proportion of the cases exhibiting one worm to a grain of flesh are thus accounted for; it is evident that this must be more than counterbalanced by the fact that there are probably many cases of *direct infection* of a type still milder than the feeblest of those here reported. From the computations which have just been made, it is clear that any case in which less than 46 females came to maturity in the intestine of the host, would present a degree of infection which might escape detection by a process of examination such as I have followed.

I think, therefore, that these inferences may be drawn: (1) That the cases of indirect infection — *i. e.*, through fæces — lie for the most part quite outside the possibility of discovery by any examination which is limited to one grain of flesh; and (2) that the cases of direct infection which are also too mild to be discovered by that standard of examination exceed those resulting from indirect infection which may have been detected, and thus have helped to swell the

total number of cases reported. If these inferences be just, the results which are furnished by my examinations certainly do not exceed the actual number of infections which have taken place as the direct result of eating trichinous meat.

The important question, then, is: What are the sources from which hogs get the infected meat? Fortunately, there are not many ways in which it is probable that dangerously infected meat could be got; *i. e.*, there are not many animals in which trichinæ are found with sufficient constancy to warrant one in supposing that they could furnish a continuous supply for infection. One of these animals is, of course, the hog itself. But there is also another, — the rat, — which, from its carnivorous, or at least omnivorous, habits, is believed by many to be responsible for the perpetuation of trichinæ in hogs. Leuckart, who was the first to call attention to the rat as a harbinger and probable disseminator of trichinæ, has given many reasons, both general and special, upon which he bases his conclusions.

But, while rats may be the principal cause for the perpetuation of trichinæ in Europe, it should not be overlooked that nowhere else in the world has so great a proportion of trichinous hogs been found as among this three thousand raised in the vicinity of Boston. I cannot believe that the conditions here are any more favorable for the perpetuation of trichinæ through rats than in European cities. I think the hogs raised near Boston are not more closely confined, and that they do not have more inducement or opportunity to kill and eat rats, than in Germany. It must not be forgotten that the most of the hogs here reported on were not raised in close quarters within the city limits, but by farmers and hog-raisers in the suburbs, at distances varying from five to twenty miles from the city proper. But, on the other hand, it is undoubtedly true, that, wherever hogs are raised, rats are more or less abundant. They are attracted by the feed which is given to the hogs, and are likely to thrive most where they have freest access to it. City offal for the hogs probably affords rats a better means of subsistence than any kind of food which is better housed and cared for than offal is likely to be. So it may be that the offal is *indirectly* responsible for the trouble, in that it serves to attract more

rats than could be supported if other kinds of food and other methods of feeding were employed.

While I am inclined to regard the infection of hogs as more probably due to their eating uncooked swine-flesh contained in the offal than to their killing and eating rats, still, an investigation, to be at all satisfactory, should not ignore this latter possibility; and it is therefore desirable that some means should be found of gaining access, for the purpose of inspection, to the hog-pens of raisers in this vicinity, and, if possible, of securing the co-operation of the raisers, in ascertaining the prevalence of rats, and whether it is common for hogs to eat them.

However true it may be, that, owing to the rats, we should never be able entirely to get rid of trichinæ in hogs, even if we could by a decree destroy every one now existing in them, still, I doubt if rats are the only or even the principal cause of the present alarming prevalence of this parasite in the pork raised in the immediate vicinity of Boston. I suspect that hog-flesh is, after all, a greater source of the difficulty than rat-flesh.

There are two principal ways in which it is possible for pork to keep up the infection in hogs. It is a custom with some private slaughterers, even with the better class of people and those otherwise well informed, to cast the viscera of slaughtered animals, swine among others, to the pigs. No surer way could be devised for perpetuating and increasing the infection with trichinæ.* At one of the State institutions this practice was kept up until January, 1887, when it was abandoned, as a result of some of the examinations here reported, and the consequent discovery of the enormous per cent. of trichinæ (26 per cent.) found in the pork raised and slaughtered at that institution. It is hoped that the discontinuance of this practice will speedily show some diminution in the number of hogs containing trichinæ.

But the acquisition of trichinæ through the viscera of slaughtered hogs is precluded in the case of those raisers

* The danger which ensues to hogs, in consuming the viscera of slaughtered animals, is not so much due to the possibility of infection from intestinal trichinæ or young embryos, as from encapsuled muscle trichinæ. Small in amount as the voluntary muscles are which are attached to the viscera (œsophagus, diaphragm, rectum, etc.) they are sufficient to constitute the principal source of danger.

who habitually send their hogs to the large private slaughtering houses of the city, and who do not receive the viscera from those institutions. Such is the practice of most of the large raisers in the vicinity of Boston, and in particular is true of all of the 56 raisers enumerated in Table III.*

The only other suspected source, aside from the rats, is the ordinary food of the hogs. In all cases, without doubt, this is to some extent kitchen offal. In the case of 51 out of the 56 raisers enumerated in Table III., city offal is known to constitute a large share of the food given to the hogs.

It may prove to be merely a coincidence, and of no importance, that hogs which are fed on city swill show a proportion of trichinous individuals far in excess of those which come from the West, among which offal-fed animals must be much less numerous than here; but it is a coincidence which at least merits a careful examination.

The probable sources of the principal part of the infection in this vicinity appear to me to be limited to these two, — rats and city offal. The only way to ascertain which of the two is the greater source of danger, is to eliminate as far as possible one of the supposed causes, *without interfering with the previously existing conditions of the other*. It will evidently be easier to control the nature of the regular food of hogs, than to eliminate the rats. With honest supervision, there need be no doubt of the completeness of the experiment in excluding all uncooked meat; but, with the most scrupulous attention, it might not be possible to secure absolute exclusion of rats, and the uncertainty in this respect would render the experiment less satisfactory than the control of the regular food.

There are also certain facts which seem to me already to point to the offal as the more probable source of the difficulty. That being the case, there would be greater hope of ascertaining the principal cause quickly, if this source were the first to be removed. One of the facts which seems to me to point to the offal rather than to the rats, is that so large a proportion of all the hogs are thus infected. It seems improbable that every eighth hog should find the opportunity

* It is a common thing, however, for some of these raisers themselves to slaughter a portion of their hogs for the purpose of supplying neighboring retail dealers.

to eat a rat, or any portion of one ; and I doubt it the more, since raisers rarely or never know of such an occurrence ; whereas, the presence of a small amount of raw lean pork in the offal — especially if 13 per cent. of all the pork were trichinous — would afford frequent opportunity for one or more hogs to become infected.

A second consideration of some weight is the probability that, other things being equal, the degree of infection in hogs acquiring trichinæ by eating rats would be more uniform and in general more thorough than in hogs infected by eating such fragments of pork as might make their way into kitchen refuse. Not that there would necessarily be any greater uniformity in the degree of infection in the rats themselves than in the pork, but that a larger amount of muscle would be consumed in the former than in the latter case.

That raw meat should find its way into the city offal is not at all surprising, even in view of the general practice of reserving for soap whatever may be serviceable as fat. That practice, it is certain, must greatly diminish the danger of infection from this source, but it cannot be sufficient to wholly preclude the admission, especially of fragments of lean meat and joints with portions of the muscle still attached.* Even a cursory inspection of the contents of offal-carts has shown me that uncooked meat finds its way into city offal ; but to what extent, it would of course be very difficult to ascertain, owing to its heterogeneous nature. The raw meat probably does not come from the butchers' shops nor from the kitchens of hotels and public restaurants, because in these places greater system prevails in the disposal of whatever may be made to yield a money return ; it is rather from the refuse of private kitchens — where it must often be a matter of indifference to the " help " whether the trimmings from a piece of pork go into the soap-grease or the offal-box — that the supply is kept up, — a supply which need not be great in amount to insure the results.

Another fact of some importance is, that nearly half (149

* It is well known that the portions of a muscle which are most thoroughly infected are the ends near their attachment to bone ; so that the fragments of meat remaining on bones are likely to be of more than average danger.

out of 351, or 42.4 per cent.) of all the raisers whose hogs have been examined have had hogs containing trichinæ. Even this statement does not give an adequate idea of the prevalence of the infection, because, in the majority of the cases where no parasites were found, the number of hogs examined was very small (*e. g.*, in 166 out of the 202 non-trichinous cases, fewer than 6 hogs were examined). Perhaps a more adequate idea of the prevalence of the parasite is to be had from the list of 56 already cited (see Table III., page 120), which embraces all the raisers who have furnished 15 or more hogs for the examinations here reported. Out of these 56 only 5 have furnished hogs destitute of trichinæ, and the number of hogs examined from each of these five raisers was respectively 17, 15, 18, 16, 16. It is, therefore, evident that no person whose hogs have been examined to the number of 19 raised pork destitute of the parasite. It is at least a striking coincidence, that, out of so many raisers whose hogs are badly infected with trichinæ, such a proportion have fed city offal.

What can be done further towards settling the question of the sources of infection? It appears to me desirable: (1) To ascertain if there are any raisers who feed brewery swill, or any other food containing no meat, to the exclusion of all kitchen offal. If there are such, to secure the examination of all the hogs they slaughter, ascertaining at the same time whether trichinous rats are to be found about their premises. (2) To make arrangements, by subsidies if necessary, with certain raisers who are known to have fed city offal and to have furnished badly infected hogs, to the end that some other feed, containing neither meat nor kitchen offal, shall be substituted for that at present used. This change of feed must, of course, extend over the whole lifetime of every pig upon which examinations are made for the purpose of ascertaining the effect of the absence of city offal. (3) To continue the examinations for trichinæ in pigs slaughtered at the State institution where the reform as regards the feeding of viscera has been effected, and where uncooked meat is now excluded from the feed, in order to ascertain what effect these changes produce. (4) To secure a more complete co-operation on the part of other State institu-

tions which raise and slaughter hogs, in order that a subsequent change in diet may afford additional opportunities to judge of the relative effects of offal feeding, and a possible consumption of rats.

EDWARD L. MARK.

CAMBRIDGE, April 16, 1888.



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